

Reproductive Potential of Delta Smelt in the San Francisco Estuary: Is the Egg as Important as the Chicken?

#0072

Technical Panel Review

Proposal Name: Reproductive Potential of Delta Smelt in the San Francisco Estuary: Is the Egg as Important as the Chicken?

Applicant Organization: University of California, Davis

Principal Lead Investigator(s):
Bennett, William

Amount Requested: \$577,484

TSP Panel Summary of Findings:

The research team has experience conducting research on delta smelt, and they pose interesting and potentially relevant questions. However, the proposal is difficult to evaluate because it is incomplete. For instance: sections are missing; methods are not fully described; figures referenced in the text are not included with the proposal; and many references are not included in the poorly organized bibliography. This incompleteness makes it impossible to evaluate the suitability of the methodology and budget. It seems that the research team was likely constrained by the proposal deadline and therefore submitted a 'work-in-progress.' However, they hint at some interesting questions and should therefore be encouraged to reapply.

Submitting a proposal which is clearly incomplete has resulted in an extensive and unnecessary expenditure of time on the part of CALFED administrators, external reviewers, and review panel members. However, this topic is highly relevant, and because of this, the panel encourages the investigator to revise and resubmit next year. This proposal team should be encouraged to undertake this work as it is foundational for the extensive modelling effort now underway.

Relevance to PSP Topic Areas:

High

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TSP Technical Rating:
Inadequate

TSP Funding Recommendation:
Do Not Fund

TSP Amount Recommended: \$0

Conditions:

External Technical Review #1

Proposal Title: Reproductive Potential of Delta Smelt in the San Francisco Estuary: Is the Egg as Important as the Chicken?

Proposal Number: 0072

Proposal Applicant: University of California, Davis

Purpose

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| Comments | Determining reproductive potential of delta smelt and how it may vary or may have declined, and thus is a contributing factor to decline of delta smelt, is a worthy goal. However, it never was clear to me how this project could answer fundamental questions related to export losses or to affect of age structure of the smelt population on reproductive success. It will provide information on condition of adults and on numbers, viability, and quality of eggs, but these measures alone contribute little to understanding delta smelt decline. The P.I.s are involved in other projects on delta smelt and in proposals to investigate and model delta smelt. It appeared to me that the research proposed here, except possibly for the culture experiments, would easily fit nicely into the ongoing or proposed projects. Better explanations of other projects would have helped me to understand the unique purpose of this proposal. Evaluating reproductive potential outside the context of population dynamics and basic dynamics parameters seems futile to me. I don't believe that the project as proposed can address the hypothesis of how export flow can alter reproductive fitness (although I think this is a good question to investigate). |
| Rating | Sufficient |

Background

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| Comments | <p>The conceptual model is 'vaguely clear.' The actual illustration (Fig 1) shows only export mortality and temperature as sources of mortality. Surely, it should include trophodynamics and other potential factors (that probably are being studied or proposed in other projects). Figures 2 and 3 are cited but did not appear in the proposal. Figures from Bennett 2005 are cited numerous times but always as Figure XX. I think that the P.I. was very pressed for time in writing and compiling this proposal. There is more evidence of this in other sections. The P.I.s state that ultimately they hope to produce stage-based population models. Results of this study might contribute some information to help with these models, but the model could not be developed based solely on this project. The idea that export mortality of delta smelt is analogous to fishing mortality is interesting and worth pursuing, but I don't think it will be in the present proposal. Having read the Background section, I concluded that this proposal is still a 'work in progress,' with much potential but it needs to be developed.</p> |
| Rating | Inadequate |

Approach

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| Comments | <p>There are three major tasks. None is described in a way that I fully understood what would be done (exception may be the histopathology). In the culture experiments, what is fed to the smelt at the various ration levels? Isn't food type (and its quality) at least as big a concern as amount? I was surprised that hundreds of delta smelt could be sampled for experiments, considering</p> |
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| | <p>its endangered status. But, presumably permits are obtainable. Task 2, egg morphometrics, is simple enough to describe with respect to measuring methods. But, what will be done with these data? Analysis? Modeling? The histopathology seems straightforward enough and potentially may identify problems with eggs. I note that on page 27 the P.I.s state that they have been doing histopathology on eggs since 2003. What did they find? What new is likely based on the proposed experiments and on more wild adult collections? The approaches and tasks as presented appear to be disjointed parts. Again, I think the P.I.s may have run out of time in preparing this proposal. I had hoped to see a much more careful description of approaches, methods, and of products (including analytical and modeling).</p> |
| Rating | Sufficient |

Feasibility

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| Comments | <p>I was uninspired and not convinced that results of this research would lead to answers to the interesting questions and hypotheses put forward. I don't doubt that they will culture females and determine how different rations affect condition (they apparently already have done such experiments), measure eggs, and do histopathology on eggs, but it was not clear in the proposal that they could answer question that are at the heart of the delta smelt decline. Will it be easy to collect >1,000 delta smelt adults for experiments? Will permission be granted to take this many fish?</p> |
| Rating | Inadequate |

Budget

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| Comments | <p>The cost of this two-year project is \$577K, which the P.I.s say is efficient and cost-effective. If it were</p> |
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| | standing alone, with no other funded delta smelt research being carried out by them, I might agree. But, I thought that the expenses were quite high, given the uncertainty (at least in my mind) of success in meeting goals. I gave this category a 'Sufficient' rating but wondered if the budget had been carefully thought out by the P.I.s |
| Rating | Sufficient |

Relevance To CALFED

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| Comments | If the research could be conducted to answer question posed, either alone or in combination with other ongoing research, it would be highly relevant. But, the proposal is not fully convincing and its relationship to other ongoing or proposed research is only vaguely explained or justified. Combined with other research, information and modeling results of CALFED projects, it could contribute meaningfully to management of delta smelt I qualify this in thinking that a better proposal is required to clearly show how it can succeed. |
| Rating | Above Average |

Qualifications

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| Comments | Lead P.I. Bennett is an expert on delta smelt. He wrote the white paper (Bennett 2005) and understands the issues and needs for research on the fish. Lindberg and Baskerville-Bridges developed methods to culture delta smelt and are likely to be successful in conducting the proposed experiment. They have an adequate facility to undertake the culturing. Teh is an expert on histopathology. Together, the P.I.s could make a good team. I would have liked to have seen a better proposal at the outset, which would have been reasssuring that the team will be functional. The proposal, as now written, seems to have been hastily pasted together. |
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| | Above Average |
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Overall Evaluation Summary Rating

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| Comments | I think the proposal is a 'work in progress.' It has potential but the version I read did not convince me that goals could be met. |
| Rating | Sufficient |

External Technical Review #2

Proposal Title: Reproductive Potential of Delta Smelt in the San Francisco Estuary: Is the Egg as Important as the Chicken?

Proposal Number: 0072

Proposal Applicant: University of California, Davis

Purpose

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| Comments | <p>This project address several important questions surrounding the lack of success that has been observed in restoration efforts for delta smelt. There is considerable literature evidence that the quality and quantity of eggs produced is influenced by the abundance, size and age composition of the adult population, and investigation into these impacts provides valuable insight into the factors potentially limiting a population. While the research goals, objectives and hypotheses seem clear, I feel that the investigators have inadequately defined the terms they use, and have developed hypotheses that are difficult to test at best. Specifically, the use of the term "reproductive potential" is not clearly defined in a sufficiently quantitative manner as to allow testing hypotheses surrounding this concept. Further, I feel the author's interpretation of their proposed hypothesis tests was not always consistent with the hypotheses stated. For example, the authors state that rejection of H1 (...smelt lost to the export facilities...) would provide evidence that "larval mortality during early spring has been inadvertently harvesting the most-fit larval and post-larval delta smelt...". My point of disagreement is that rejection of this hypothesis only provides</p> |
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| | evidence of differential loss of larvae, and not the most fit. It could readily be the case that the larvae of lesser fitness are the ones lost. |
| Rating | Sufficient |

Background

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| Comments | <p>In general, the proposal provides a reasonable basis for the proposed research. Two areas were problematic, however.</p> <ol style="list-style-type: none"> 1. Much of the justification is based on "personal observation" or other poorly documented phenomena. A critical unknown the authors identify is that the actual spawning locations of delta smelt are unknown. As someone outside of this region, it seems to me that the lack of this knowledge undermines all of the conceptual model to the extent that the processes influencing early life history would be unknown because of the lack of knowledge of where fish are spawning. 2. A substantial part of the proposal focuses on the use of histopathological methods, but I did not see any clear rationale for the use of these methods, nor a conceptual model of how results from these analyses would be incorporated into the overall project. 3. An area that has frequently troubled me in the conceptual basis of studies like this one is that small fish often are observed to have smaller eggs (presumably producing larvae of lower fitness) than larger females. The fundamental question I pose is "Why?", and what conceptual model could explain this phenomenon. Why would a small fish produce eggs that presumably (underscore word presumably) produce larvae doomed to mortality? Why would they not produce fewer but larger eggs that are able to compete with the eggs from larger females? I don't expect the authors to answer this question, but I feel it is a major gap in |
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| | the conceptual basis of this and similar studies. |
| Rating | Sufficient |

Approach

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| Comments | <p>I have a number of concerns that I feel are fatal flaws in the overall design of the study. The main concerns are:</p> <p>Task 1. The experimental design proposes to maintain 880 fish in four tanks, two at 1/2 satiation feeding, and the other two with feeding at satiation. My main concern is that the fish within a tank are not independent (underscore word independent). This has several implications. First, the true level of replication is two tanks per treatment, a level that I feel is inadequate to answer the questions posed. Secondly, the performance (e.g., growth) of fish within a tank depends not only on feeding levels, but on the density of fish within a tank, which generally declines at different rates within different tanks or treatments. Analysis of the data collected under the proposed design would likely have an inherent confounding of fish density and feeding level. As such, it would not be possible to interpret the results of such an experiment cleanly. I recognize that designs as proposed have been used in the past, and have been published, but I feel that investigators have ignored the fundamental problem of lack of independence which is a critical assumption of analyses typically used for such experiments. My recommendation would be to place individual larvae in small aquaria to increase replication and provide independence. Practicality may require having multiple larvae within an aquarium, but I strongly recommend developing protocols and approaches for maintaining equal density across treatments over time.</p> <p>Task 2. No rationale is given for collecting data on external morphometrics of adult delta smelt. No details are given on the specific data to be</p> |
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collected, the sampling design to be used, the analyses proposed, nor on potential interpretations of anticipated results. For example, how will data on external morphometrics be summarized (e.g., will a truss network be used, will principal components be used, or will another summary be used) and more importantly, how will this information be used to inform the investigators about reproductive potential under their conceptual model?

Task 3. As identified above, the rationale for histological analyses is not clear to me. A fundamental conceptual flaw in this task is that the investigators state that they will "develop indices of reproductive potential" when they have not clearly defined how they will measure reproductive potential in an absolute sense. By analogy, to demonstrate the reliability of an index of relative abundance of a fish population, one show how it is related to the actual abundance of fish within a population over time, or across several populations. Without a measurement of true abundance, one is left having to assume the index is reliable.

Another major flaw (in my opinion) is the reliance of egg diameter as a measure of egg quality. My understanding of the biology of fish eggs in general is that the size (diameter and volume) of eggs vary as a function of the nutritional resources stored in the egg __and__ the level of egg hydration. This process is particularly well documented for marine fishes, but has been observed for freshwater fish as well (e.g. in rainbow trout, Milla et al. 2006. Hydration of rainbow trout oocyte during meiotic maturation and in vitro regulation by 17,20 dihydroxy-4-pregnen-3-one and cortisol. Journal of Experimental Biology 209: 1147-1156). If the level of hydration is not considered, this would seem to be a critical misinterpretation of egg diameter and volume as a predictor of the fitness of the larvae that would be produced. This is not an area of my expertise, but

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| | even with my naivet  , I recognize this to be an important issue, and would have expected the investigators to address this point. |
| Rating | Inadequate |

Feasibility

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| Comments | The approach proposed is likely feasible, but given my concerns listed for the approach, the issue of feasibility is really relevant. In terms of the documentation provided, I would note that the proposal had numerous sections that were apparently incomplete. Examples include figures cited as XXX, and having an incomplete and poorly prepared literature cited section (I found references in the proposal that were not cited, and the citations are not presented in alphabetical order). |
| Rating | Sufficient |

Budget

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| Comments | The budget appears reasonable for the work proposed, but as I indicate in the approach comments, the necessity of some of the work proposed is not well justified. |
| Rating | Sufficient |

Relevance To CALFED

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| Comments | The topic of the work proposed is relevant to CALFED's goals and objectives. |
| Rating | Sufficient |

Qualifications

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| Comments | The authors are clearly well qualified investigators who have a proven track record of successfully initiating and following through with projects of this scope and scale. As such, I feel that a suitable |
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| | revision of the proposal to address the concerns I've listed would result in a project that would be superior given the investigators' abilities. |
| Rating | Superior |

Overall Evaluation Summary Rating

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| Comments | Overall I feel the issues surrounding the proposed approaches are serious enough to warrant an overall rating of inadequate. The topic of this research is very relevant and scientifically exciting, and with suitable revision, the research would likely rate very highly. |
| Rating | Inadequate |

External Technical Review #3

Proposal Title: Reproductive Potential of Delta Smelt in the San Francisco Estuary: Is the Egg as Important as the Chicken?

Proposal Number: 0072

Proposal Applicant: University of California, Davis

Purpose

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| Comments | <p>The goals of this work are stated relatively clearly in the proposal, and appear to be reasonable based on prior work. Based on my overall evaluation of the proposal, I would suggest that the proposed work <i>*could*</i> lead to useful information. However, there are a number of questions relative to the proposed work that lead me to question the contribution that this work would eventually make should it be funded. In addition, the overall preparation of the proposal was simply not up to the expectations that one would have for a request of more than a half a million dollars. While this does not necessarily translate into the actual work that is being proposed, it certainly does detract from the overall picture of the proposed work. The abundance of typographical errors, mistakes in the literature cited section, missing references, missing figures, references to figures as XX, etc. lead me to question how much time, effort, and thought has gone into the planning and preparation of this proposed work.</p> <p>Although the objectives (n=5) are stated explicitly in the proposal, it is not at all clear how three of the five objectives are related to the goals as stated. In particular, the proposal does not make clear how the second, fourth, and fifth objectives fit in with the two goals that are stated. In addition, there is an alternative hypothesis (H1) stated, but there is no null hypothesis against which this alternative</p> |
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| | <p>hypothesis can be tested. So it is not clear whether the H1 is really a null hypothesis or whether it is truly an alternative to something else (that is not stated).</p> <p>The purpose of the proposed work appears to be justified in large part on Bennett (2005), which is cited as being in the journal Estuary and Watershed Science (which I assume is really San Francisco Estuary and Watershed Science despite the incorrect journal name and a lack of page numbers in the literature cited section!). Although this is a published paper, it is not easy for reviewers to obtain, and is difficult to read as background for the current submission, given that it is 71 pages long!</p> <p>In sum, components relative to the purpose of the purpose are included, but they do not fit together into a cohesive unit, and thus the overall description of the purpose of the proposal is lacking.</p> |
| Rating | Inadequate |

Background

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| Comments | <p>Based on the proposal, the PI has done a reasonable job of describing the situation with Delta smelt that may have led to or may be maintaining their current population status. As I understand the situation, large smelt spawn relatively early relative to smaller fish, exporting flows are greater early in the season, which leads to the possibility that the early-spawning fish that are more robust may be more likely to be exported from the system. However, the figure that is presented as a conceptual model to support this scenario is described very poorly in the text and in the figure title, making interpretation of the PI's depiction of the system very difficult. The figure contains three columns, and it is not clear how those columns interact, why there are abundances in the process column, how export mortality relates to the environment they are in, what the words are that are</p> |
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| | typed on top of one another at the bottom of the middle column (and why they are both there), etc. In addition, there are a number of citations throughout the background section of the proposal that are not included in the literature cited section, there are figures cited in this section as "Figure XX" or "Figure XXX" that are not included, there is a citation to a web page that is listed simply as "URL?". Again, the sloppy preparation makes the quality of the background difficult to evaluate. |
| Rating | Inadequate |

Approach

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| Comments | <p>The approach to be used is divided into three tasks. The work to be conducted is described in moderate detail for Tasks 1 and 3, but is not described in adequate detail for Task 2. {Note that Task 3 is defined as having two different titles-- one that is the same as Task 1 and one that is different. Although it is fairly obvious that Task 3 is supposed to be different from Task 1, this is another example of sloppy proposal preparation}. Task 2 is described by a total of two (2) sentences and is defined to consist of counts and measurements with no detail as to what is being measured and/or counted, what sample sizes will be, what is the timing of collection, how will data be analyzed, etc. It is not at all clear how any data collected via Task 2 will address any of the 5 objective questions. In addition, there is no description of what will be measured in the experiments to be conducted in Task 1, nor how the year-to-year variation will be incorporated into any analyses.</p> <p>It is clear who will be responsible for the administration of the project, as well as for conducting each of the three tasks.</p> <p>The dissemination of the results of this work is extremely limited, being in the form of presentations</p> |
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| | and only (a minimum of) 2 peer reviewed scientific publications. The outlets for presentations seems reasonable, particularly given the topic and funding source. However, in my view, planning up front to publish only a minimum of 2 publications from more than a half a million dollars of funding is simply inadequate. |
| Rating | Inadequate |

Feasibility

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| Comments | While it may be feasible to ask the questions that are laid out at the outset of the proposal, the tasks do not explicitly address those questions, nor are the tasks described in sufficient detail to determine whether the work is technically feasible. As such, given the proposal as presented to me, feasibility cannot be assessed. |
| Rating | Inadequate |

Budget

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| Comments | The budget is similarly not defined in terms of the tasks that are defined in the proposal. For example, in the budget portion of the proposal, task 1 is defined as the administrative portion of the project, yet it should be the 2 year experiment. This should likely be the most expensive portion of the work, yet this task is budgeted for only \$36K of the proposed \$577K. In addition, in general I do not see that \$577K is required for the work as it is described in this proposal. This may be due to an inadequate description of the work, but I cannot determine this at this point. |
| Rating | Inadequate |

Relevance To CALFED

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| Comments | The work that is described in the proposal does appear to be highly relevant to the priorities as stated in the PSP. This is the one area in which the PIs have done a good job of showing how the proposed work would fit in with the goals of CALFED. However, again due to the lack of detail and adequate preparation, it is not clear that the proposal would actually lead to the type of results that the PIs suggest. As such, it is not at all clear whether the information would ultimately be useful to CALFED resource managers and/or policy makers. |
| Rating | Sufficient |

Qualifications

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| Comments | The PIs are clearly well qualified for the work as they describe it in the proposal. They have collectively worked in this system for several years, and have a good deal of experience with this system and this species. I am confident that if the PIs were to better define and describe what they want to do in a proposal, they could conduct such work. |
| Rating | Sufficient |

Overall Evaluation Summary Rating

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| Comments | This proposal was poorly prepared, and the work was not described adequately for a reviewer to be able to determine whether the stated goals were the logical next step, whether the objectives would allow the PIs to achieve their goals, and whether the methods would even address the objectives/questions being asked. Goals and objectives that do not match, missing figures and literature in the background, and inadequate details relative to the methods all lead to the conclusion that this proposal should not be funded. A revised proposal might be fundable, but this one is simply too far away from that point to |
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| | determine at this point. |
| Rating | Inadequate |

#0072: Reproductive Potential of Delta Smelt in the San Francisco Estuary: Is...

External Technical Review #4

Proposal Title: Reproductive Potential of Delta Smelt in the San Francisco Estuary: Is the Egg as Important as the Chicken?

Proposal Number: 0072

Proposal Applicant: University of California, Davis

Purpose

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| Comments | The purpose of this project is to explore unknown aspects of the reproductive biology of delta smelt and how water withdrawals may be acting as "fishing" to select against the most viable larval fishes. The idea is both timely and important to the management and restoration of this threatened species. I liked the proposal in concept, but found it lacking in justification (much of this work has been examined in earlier studies referenced in Bennett 2005, so we are not sure whether the proposed study really expands upon this work or if it is needed. Greater presentation of the Bennett work is needed here with differentiation of how the proposed work extends upon it). The proposal refers to many figures that are either missing, or in the Bennett 2005 paper but without either at my disposal it was impossible to definitively determine how much this proposal is needed and how much new information would be generated. Assuming this is novel research it should be an important consideration in modeling efforts related to delta smelt. |
| Rating | Sufficient |

Background

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| Comments | The underlying basis for the proposed work is fairly well laid out. This is probably the strength of this proposal. It clearly states the steps in the lines of reasoning that lead the researchers to propose this |
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#0072: Reproductive Potential of Delta Smelt in the San Francisco Estuary: Is...

External Technical Review #4

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| | study. However, the first signs of sloppiness in the preparation of the proposal emerged in this section with numerous references to "see Figure XX" or "Figure XXX" listed in the text, missing references e.g. "()", and orphaned text that does not appear to relate to the section (e.g. "Preliminary versions" at the bottom of one page. Obviously these types of errors should have been corrected before submission! |
| Rating | Sufficient |

Approach

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| Comments | <p>The approach also fails to fulfill the promise of this proposed study. The methods section consists of a series of bulleted text that essentially reads like one of the proposal preparer's notes on the study plan. Again, missing references to smelt being "extremely fragile ()." The experimental design is not sufficiently described, including the justification for the sample size (n=2) in the tank experiments. It is very unclear what or how things will be analyzed (statistically) in order to meet objectives or how extension to field collections will be made. There are two things listed as "task 3" with one not having anything listed under that heading and additional parts reading like someone's notes for preparing the proposal (e.g. "Teh's task"). Overall this is the weakest part of the proposal. I must say I was quite disappointed with the care taken in putting together the proposal....too many mistakes.</p> <p>One thing the PIs may wish to address is how temperature and the interaction of temperature and ration influence timing of spawning and fecundity. These would help to better address how well delta smelt will deal with environmental factors.</p> |
| Rating | Inadequate |

External Technical Review #4

Feasibility

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| Comments | From what I can tell from the poorly defined methodology I think the proposal would be technically feasible. The likelihood of success is difficult to assess. On the one hand the research team appears to be a strong one with lots of valuable experience in this area. However the many errors and mistakes in the proposal must make a reviewer wonder is similar sloppiness and errors would occur in carrying out this research. Provided these mis-givings I think the scale of the project and the objectives should be within the grasp of the authors. |
| Rating | Sufficient |

Budget

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| Comments | <p>In my review of the budget I think it might be overpriced. For a two-year study that is piggy-backing onto an existing field program for collecting smelt for fecundity and egg quality estimates I think \$190K is too much. Costs could likely be curbed by using student workers or grad students for much of the lab and field work with oversight by the PI's. A PhD is not required for this work with proper oversight.</p> <p>The \$351K budget for two 8-week lab experiments is even more overpriced. I would need to see a much better rendering of the methods and timelines in order to conceive of these experiments costing this much to conduct. Maintaining four tanks of fish for each experiment should not be this costly.</p> |
| Rating | Inadequate |

Relevance To CALFED

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| Comments | The proposal is well-related to the priorities of the PSP. The proposal will address two PSP priorities-- populations trends of delta smelt and ability of the population to respond to environmental change. |
| Rating | Inadequate |

Qualifications

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| Comments | The research team has the right background and credentials to be able to carry out the study. |
| Rating | Superior |

Overall Evaluation Summary Rating

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| Comments | My overall rating of this proposal was "inadequate". I believe the reason this proposal was in this category was because it appears the PIs were rushed to put the package together by a deadline and as a result the submitted document lacked clarity and completeness in the methodology and contained too many sloppy errors. I provided several examples of such sloppiness errors above, but there were many others. Even the literature cited was put together haphazardly and not in alphabetical order. I really wanted to rank this proposal highly because the concept is good and the research is timely and deals with a critical species in the estuary. However, for the reasons outlined above I could not rank it higher or suggest it should be funded. I would encourage the PIs to spend time cleaning this proposal up and providing better methodological detail and better budgetary accounting and ask them to resubmit at a later time. |
| Rating | Inadequate |